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Consequently, my researches on annular vessels afford opposite results to those of Schleiden. Nevertheless, I am far from pretending that he has observed badly; on the contrary, he appears in these researches as a skilful phytotomist, and as one experienced in the use of the microscope; but I think his interpretation of what he saw to be far from exact, inasmuch as he has considered accidental though persistent variations of form as normal, transitory and necessary stages of the metamorphosis.

V.—*Preliminary Reply to Mohl's Essay on the Structure of Annular Vessels.* By Dr. M. J. SCHLEIDEN*.

M. MOHL has answered my challenge more speedily than I could have expected, and published his remarks in opposition to my late treatise. These relate only to my views regarding the origin of annular vessels. It is to two points especially that I purpose at present to call attention.

I am almost ashamed to mention the first, viz. Mohl's correction of my notion, that the crossing of the pore-like fissures depends upon the apposition of oppositely turned spirals. The thing is so simple, that I scarce understand what demon of thoughtlessness possessed me when I entertained the notion; and, in spite of the mortification of being caught in so marvellous an hallucination, I thank him from my very heart for having in so friendly a way rid me of this goblin. I should scarcely deem it necessary to mention the matter at all did I not consider it the indispensable duty of every conscientious inquirer to retract openly and expressly every known error, as the so doing is the only surety that he is indeed anxious for truth.

The other point concerns the main subject itself. The history I gave of the development of annular vessels respected only the simplest case, that of rings arising from a single thread; and I feel confident that I have not deceived myself in the cases alleged, since my researches were made on ves-

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sels which, when mature, are purely annular; so that I could not but believe that I had before me not mere persistent modifications of structure, but really stages of transition, even though I could not have regarded the observed forms as actually detected in the act of development; not to mention, among other circumstances, that the persistent ring is distinguished by the sharpness of its outline, the firmness and clearness of its substance, from the yellowish gelatinous transitory portion with its eroded and defaced margin observed in the moment of dissolution. I trust now that Mohl himself will be convinced of the rectitude of my assertions in these particular instances.

As regards the other forms, as they are figured by Mohl f. 1—6, 10 (Pl. I.), they do not come under the notion of annular vessels as defined by me in my treatise, but under that of reticular formations, whose reference to one or more deformed spirals is as easy or even easier than in porous organizations.

But, in general, I might pronounce the conclusion as to the mode of development from the perfect form as highly improper, for it cannot have escaped Mohl, that, after the formation of the original spiral, in many cases secondary threads are developed as members of union, which consist of quite a different substance, since they are soluble in boiling alkali, yet apparently do not differ from the spiral, and make the perfect comprehension of the fundamental spiral extremely difficult. I consider the part of Mohl's figure 10 marked (*a*) as of this nature. The formation of such secondary threads is frequently observable in reticulate vessels and in some forms of scalariform vessels. They occur, however, in the most remarkable degree in the large purely spiral vessels of the stems of *Scitamineæ*, as in *Hedychium coronarium*, *Canna*, &c., when on their gradual decline they are filled with cells. Such a peculiar luxuriance of the threads then takes place that the originally pure spiral vessel is only distinguished from a porous vessel by the perfect regularity of the pores. Moreover what moves me especially to adhere still to my views, is the philosophic necessity, in a faithful investigation of nature, to limit the number of principles of interpretation so long as the impossibility of referring a phænomenon to an old principle does not imperatively require a new one.

As such in point of fact, as respects the present state of science, must I now freely regard Mohl's discovery of the primary development of annular organisms; and nothing now remains but to let both modes of origin stand separately by each other. I by no means, however, think that such will always be the case. The conciliation of this schism will then

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only be effected, when the solution of a far greater puzzle, viz. the construction of the spiral and its peculiar genesis, shall be achieved; and I would beg Mohl to direct his attention to this, as I myself have done long since. Heartily should I rejoice with him should it fall to his lot to solve this problem as he has already done in so many other cases.

VI.—*Report of the Results of Researches in Physiological Botany made in the year 1839.* By F. J. MEYEN, M.D., Professor of Botany in the University of Berlin.

[Continued from vol. vii. p. 471.]

On the Evolution of Heat by Plants.

A VERY beautiful series of experiments on this subject has been published by MM. G. Vrolik and W. H. de Vriese*; they have continued their researches on the evolution of heat in the spadix of *Colocasia odora*; they were published at the end of 1838, but we received the journals too late to be able to insert them in our former Report.

The above-mentioned observers express their astonishment at the explanation given by M. Raspail of the evolution of heat in the spadix of the *Aroideæ*, but add that their new observations were not made in order to disprove Raspail's view, for that is not necessary. The first observations were made with the spadix of *Arum italicum*; they were made in the open air, and no rise of temperature was observed: in the interior of an orangery another flower exhibited a considerable increase of warmth, and also when the light was shut out and the spatha removed, still an increase of temperature took place, as was to be expected. Moreover experiments were made with the spadices of *Colocasia odora* under similar circumstances, both when the spadix was cut away and when only turned back; the maximum difference between the temperature of the air and the interior of the spadix was $19\frac{3}{4}^{\circ}$ Fahr.

Moreover interesting experiments were made on the phenomena exhibited by the spadices of *Colocasia* in different gases, for which purpose a very excellent apparatus was contrived. The rise of temperature in two perfectly similar spadices which happened to be in perfection at the same time was observed, one in the common air and the other in the above instrument in an atmosphere of oxygen. The latter in

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